On the Structure *and* Mode *of* Formation *of the* Dental Tissues, *according to the* Principle *of* "Molecular Coalescence." By George Rainey, M.R.C.S., and Lecturer and Demonstrator of Surgical and Microscopical Anatomy at St. Thomas's Hospital.

In a paper contained in the 'British and Foreign Medico-Chirurgical Review,' for October, 1857, "On the Elementary Formation of the Skeleton," I have intimated that the globular form of dentine noticed by some of the later writers on the dental-tissues, is the result of the same process of molecular coalescence as that by which bone and shell are formed; and that the so-called "dentinal tubules" are merely spaces bounded directly by the dentine-fibres, and the partially coalesced dentine-globules. As, at that time, I was not aware that a similar view of the nature of these passages had been published by M. Raschkow, any observations of mine, however demonstrative of the fact, can only be regarded as confirmatory of his. This view of the nature of these passages had, to my knowledge, no supporters among those who had written upon the subject in this country, being supposed to have its origin entirely in the fibrous appearance presented by dentine under a low magnifying power, and thus Raschkow's view was passed over as incorrect. I may remark, that as there are few structures which have been so minutely and carefully examined as the dental tissues, it must follow, that so far as microscopical appearances are concerned, it will be impossible to add much to what has, with more or less minuteness and accuracy, been described, and therefore I shall have but little to communicate strictly of an anatomical character which will be new. But originality in this respect is not, in this communication, my object, which is to give the proper interpretation of appearances already described, and to show that they admit of being accounted for, and their mode of formation demonstrated on the same principle of molecular coalescence as has been applied to the formation of shell-tissues. I may further notice, that as this article must be as brief as possible, only such details of the structure of the dental tissues, and the organs concerned in forming them, will be introduced, as are absolutely necessary to render the physiological observations and explanations intelligible.

In discussing the subject, two descriptions of parts will require to be considered; namely, the tooth itself, and the

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organs concerned in its formation. The latter comprises the dentine-pulp, the enamel-pulp, and the osteo-dentine or bone-pulp. These are all composed of areolar tissue, vessels, and nerves, and are provided each with an epithelium. The former consists of a hard part, made up of dentine and enamel, and a soft part. This latter is limited to the immature organ, and, having the same relation to the calcified portion of a young cusp that the membranous edge of a flat bone has to the ossified part, I shall call it the membranous matrix of the cusp. This being the part where the process of calcification commences, and on which the progressive stages of that process admit of being easily examined, it will require to be described with some degree of minuteness. And to make this perfectly intelligible, a general view must first be taken of all the parts concerned in the formation of a cusp, and of the cusp itself. This will be best done by referring to the following diagram (fig. 1), which is intended to represent all the parts as they are found in the ossifying cusp of a mammal before the tooth has passed through the gum.

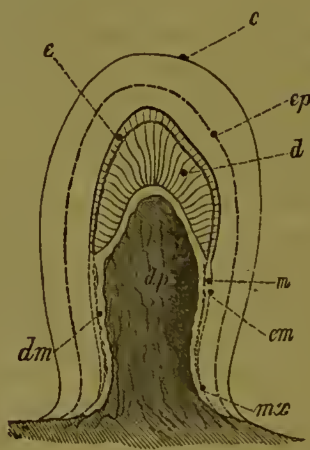

Fig. 1.

c, capsule; *e-p*, enamel-pulp; *d-p*, dentine-pulp; *m*, matrix, undulated, and dividing into *e-m*, enamel-matrix, as the external, and *d-m*, dentine-matrix, as the internal layer; *e*, enamel; *d*, dentine.

A cusp, in which the process of calcification has made but little progress, is best adapted for this examination. In such a cusp, when seen by a low magnifying power, a mere shell of tooth-substance, with a membranous border extending from its lower margin—the membranous matrix—is distinguishable. (Pl. XII, fig. 4). The relative proportions of the hard and soft parts will vary as a cusp approaches the

state of a perfect tooth, the latter retaining nearly the same width until the process of calcification is completed. However, I may notice, that in a cusp about one eighth of an inch in length, taken from a fœtal calf, the membranous border constituted at least a tenth part. This structure, when all the parts of an uncut tooth are *in situ*, is situated between the enamel and dentine pulps, its surfaces being respectively in contact with the corpuscles of each and its free margin lodged in the groove formed by their union, to which groove it is united by exceedingly fine connective tissue. The surface of the membranous matrix is not smooth, but pitted, and the lateral borders appear to be undulating, these presenting the same irregularity of form as that presented by the surface of dentine which is in contact with the rods of enamel. Before calcareous particles begin to be deposited on the membranous matrix, it has no appearance of being divisible into layers; but afterwards its division into two layers can be demonstrated, one layer following the lower surface of the layer of enamel in progress of formation, and the other the corresponding surface of the incipient layer of dentine. Its presence in these situations after a time becomes obscured by its intimate connexion with these tissues.

In fact, one layer may be regarded as the membranous matrix of the dentine, and the other as the membranous matrix of the enamel. But this will be better understood when the process of calcification is considered. As respects the structure of this part, it seems to me to be more allied to cartilage than to any other description of tissue, although, in its anatomical characters, it differs materially from it. The membranous matrix appears to be made up of very delicate flattened corpuscles of different shapes and sizes, but generally longer in the vertical than in the transverse direction of the cusp. Near its lower part these corpuscles are imperfectly defined, and in all parts of it they are partially separated by spaces more or less distinct in different cusps, and in different parts of the same matrix. The matrix, when *in situ*, being contiguous to the dentine and enamel organs, has, after its removal, frequently patches of their corpuscles left upon it, which, from their distinctness, and the regularity of their form, cannot be mistaken for those of the matrix itself.

Having now described this part with some degree of minuteness—though not, I conceive, more so than is commensurate with its physiological importance, inasmuch that it not only presents the earliest conditions both of dentine and enamel, but is also the part on which the process of

calcification can be examined with the greatest facility—I shall proceed to consider the process of calcification and the mode of formation of the hard parts of a cusp; and shall first speak of the structure of dentine and its mode of formation.

As the structure of this tissue owes its histological characters to the manner in which it is formed, the account of the mode of its formation will best precede that of its structure; and, therefore, I shall give first the process by which it is formed. The first microscopic indication of the presence of dentine is the appearance of very minute, and more or less scattered, bright particles on the inferior surface of the membranous matrix, a short distance from its lower border. They are far too minute to admit of accurate measurement, appearing merely like very fine particles of dust (fig. 3 *b*). Examined nearer to the hard part of the cusp the dentine-particles are seen to be larger, and of a more or less rounded form, and, in the cusp of the fœtal calf, to be arranged in lines of partially coalesced globules (fig. 3 *c*), but at the free border of a half-ossified fang of human tooth they are collected into globular masses. The component globules of which being, as in the lines, only imperfectly coalesced, spaces, generally considered as tubules, are left between them. Afterwards, a still further coalescence taking place, the lines, which before consisted of strings of globules, are now become long fibres, or rods of dentine. Now this process, in point of principle, is exactly the same as that which takes place in the calcification of the claw of the lobster, of which any one can convince himself who will take the trouble to examine that part in the proper manner. The intervals between the rods, if traced backwards, will be seen to end in, or rather to become continuous with, those between the larger rounded portions of dentine, and these, with the interstices between the smaller granules; and thus these spaces diminish in size, but increase in number, until they are lost among the dusty-looking particles first described. These spaces, severally situated either between the rods of dentine or between the partially coalesced dentine-globules or granules, are the so-called "dentinal tubules," which, by writers upon the dental tissues, are said to have appreciable walls or parietes. As I believe that the intervals in question are merely spaces between the uncoalesced rods of dentine, exactly like those between the rods of enamel, I shall proceed to give my reasons for entertaining this view.

And this being the negative side of the question, I may adduce the following argument. Now, as during the entire

progress of formation of all the so-called "dentinal tubules," a portion of dusty-looking material—incipient dentine—always intervenes between the partially formed tubules and the dental pulp, all the fluid which is contained in their interior, must have first passed through mere interstices or spaces. Hence if at this, the most important epoch of a tooth's formation, mere spaces have sufficed for the conveyance and supply of interstitial fluid to its substance, I do not see why parietes should be afterwards added to those spaces; as by such an addition a complex form of structure would be superadded to a simple one, after the tooth-tissues had ceased to perform any obvious organic function, their office being purely mechanical; and thus this substitution of tubes with parietes for mere spaces would come too late to serve any obvious purpose. In other parts where tubes exist, as in the tracheæ of insects, or ducts of glandular organs, a function is performed entirely distinct from that which was required to build up these parts—and one obviously requiring such a system of tubules. But the spaces between the dentine-fibres and between the dentine-globules do not come under this category, but appear to be merely a form of interstice, suited to the character and form of the tissue in which they exist, and so to be strictly analogous to the spaces between the rods of enamel.

As respects the proofs resting upon facts apparent from the examination of dentine by the microscope, I am convinced they are sufficient to satisfy any one who will examine the subject with impartiality. It may not be out of place here to state that the microscopic examination of the dental tissues is far more easy than is generally supposed. For the successful investigation of this subject fœtal teeth, perfectly fresh, are indispensably necessary, and these can be obtained almost at any time. I have especially examined those of the calf as being most easily procurable. It will not be necessary that the investigator should grind and polish sections of all the teeth which he examines; however, it will be advisable that he should possess two perfect sections, which can be easily procured from the opticians. I make these remarks rather to encourage those who are afraid to undertake the subject from the supposed difficulty of making suitable preparations, than to dissuade any one from using his own fingers. Sections of decalcified dentine, examined in glycerine, are very useful, and can be made without any difficulty. It has been stated, that even and regular sections cannot be made of teeth softened by an acid. This does not in the least agree with my experience. A very convenient way of decalcifying

teeth in the shortest space of time, and with the feeblest acid, is to suspend them by a thread in a moderately large quantity of the acidified fluid. One part, by measure, of hydrochloric acid, with twelve of water, will serve very well for this purpose; but the solution may be weaker if preferred. Globular dentine of the human tooth, as observed by Mr. Salter, may be easily obtained by introducing the point of a penknife into the hollow of a half-grown fang and scraping its inner surface; or, which is better, by chipping off the free edge of the opening into the fang.

The previous decalcification will not be applicable to enamel, which, containing so little soft material, is lacerated and torn into pieces by the effervescence occasioned by the action of the hydrochloric acid upon the carbonate of lime it contains. However, this tissue is well seen in young cusps, as will be shown hereafter. For the examination of the true nature of the so-called dentinal tubules, the sections of the decalcified dentine must be of different kinds; some being parallel with the pulp-cavity, and others at right angles to it. In such sections, when compared with similar ones of teeth not decalcified, it will be seen that the form and bulk of the decalcified dentine-rods and -globules are not altered, and that they are only distinguishable from those of the perfect dentine by an inferior degree of brightness. This gives an advantage to the decalcified specimens when employed for microscopic purposes. The exact form, extent, and precise situation of the dentinal interspaces are best defined in these, in consequence of the lower refractive power of the material by which they are surrounded interfering less with accurate definition. By such a mode of procedure it will be seen that dentine is made up of solid rods or fibres of a quadrilateral figure (fig. 6) running in different directions from the pulp-cavity towards the external surface, some being parallel with, others at right angles to that cavity, and a third set passing in all the directions intermediate between these extremes. At each of the four angles of all these rods a space, or so-called tubule, exists (fig. 6 *b*), being formed by the meeting of the four adjacent angles of the four contiguous rods (fig. 6 *a*). This interval is more or less limited to the point of conflux of these rods in different parts of a tooth, the difference depending upon the degree of coalescence of contiguous rods. Sometimes it extends some distance between each two adjacent rods, whence the appearance of a dichotomous division of a space is produced. On the contrary, in other parts the coalescence of the adjoining fibres or rods is so complete, that not only the spaces between the apposed surfaces of the

rods, but also the intervals at the junction of their angles, are obliterated. In such cases dentinal tubules are said not to be present. Now when a thin section is made through such an assemblage of fibres and passages as above described, and as represented in Pl. XII, fig. 6, the cut rods will present sections of various forms, some will be nearly square, others diamond shaped, and a third set linear; these forms depending upon their several directions — and from what has been before stated concerning the directions of these fibres, it is obvious that the forms will gradually pass one into the other.

The subjoined diagram (fig. 2) representing a vertical section of a tooth at a more advanced stage than the former

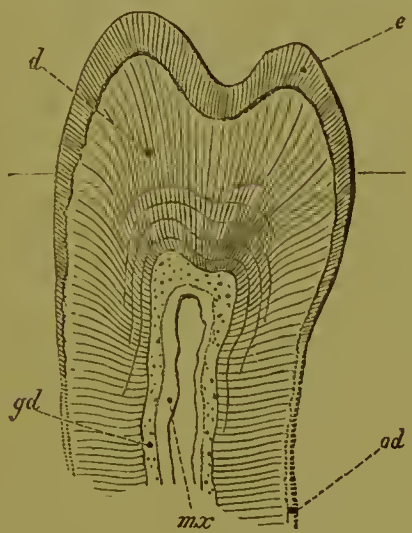

Fig. 2.

e, enamel; *d*, dentine; *o-d*, osteo-dentine; *g-d*, globular dentine; *m*, matrix. The horizontal line represents the part from which the section, a portion of which is figured in Pl. XII, fig. 6, was taken.

one, will illustrate the various points spoken of, as well as the reason for the different forms on section of dentine-rods.

Now if these rods had inclosed tubes of the form represented in plates intended to show them, their sections must have presented first circular areas, and then ovals, becoming gradually more and more excentric until they ended in straight lines. But such is not the case; the sections of these spaces, at first more or less circular, become angular or arrow-shaped, the before-mentioned lines diverging from the angular point losing themselves between the contiguous rods, the depths to which these lines extend depending upon the amount of their

coalescence. Where the rods are imperfectly formed, being made up partly of globular portions, then the passages running between them will partake of the same form, and an appearance of anastomosis will result. If the fibres should take a flexuous course, then the contour of the quadrilateral areas will be more or less curvilinear. In fact, it is certain that, if a body were made up of square rods placed side by side, and so inclined towards one another as to assume all directions between a vertical and horizontal axis, and if, at the conflux of every four, there was a minute space, which passed more or less deeply between each adjoining pair, such sections made through this body, as have been directed to be made through the decalcified dentine, would exhibit the same appearances.

The appearance presented in transverse sections of dentine, of rings with a dark point in the centre, has been too exclusively regarded as satisfactory proof of the existence of distinct tubes. It is well known, however, that this appearance is delusive, and not to be depended upon. In proof of this I have only to adduce the structure of the silicious cuticle of the common cane, and the appearance which it presents under the microscope. This is best seen in cuticle which has been boiled in nitric acid. The structure is made up of hexagonal blocks of silica (fig. 1 *a*, *b*), each block having within it a flask-shaped cavity, with the narrow part uppermost (fig. 1 *c*); and at the conflux of every three such blocks there is a space extending from its superficial surface down to the layer of cellular tissue upon which the portions of silica rest. Now these spaces, of the true nature of which no one who has examined them can entertain a doubt, present exactly the same forms and appearances as have been described in the dentine, namely, the annular, arrow-shaped, and linear forms, according to the direction and position in which they are viewed. Also a too great anxiety to account for all appearances on the cell-hypothesis has contributed much to the idea of the tubular nature of dentine, and thus these imaginary tubules have been attributed to "certain filiform prolongations of dentinal cells," or to their elongated nuclei. For between cells and tubes there is considerable analogy, so that an erroneous idea originating in the one is easily propagated to the other. In this manner, probably, the fibres of the crystalline lens have been thought to acquire a tubular form, and hence these also are now described by Professor Kölliker as tubes—an error which will be seen to be corrected in my account of the structure and development of that organ. But the chief cause of fallacy on this point is to be traced to the erroneous notion generally entertained of the

supposed physiological importance of the soft material which is left after the action of acids on hard structures; these residua having been regarded as the formative organs and receptacles of the removed earthy matter. Hence has originated the idea of different kinds of dentine, as "well-formed consistent dentine," "secondary dentine," "tubeless and uncalcified dentine," according to the relative quantities of earthy matter thought to exist in combination with the soft tissue.

Now I have no doubt but that the whole of this is erroneous, and that there is only one kind of dentine which, even in its molecular state, is as perfect as it is in the so-called tubular dentine, the latter being formed by the coalescence of the particles of the former, exactly in the same way as the larger globules of earthy matter occurring in the deep layers of the shells of Crustaceans are formed by the coalescence of the smaller ones.

As for "uncalcified dentine," I know no other part of a calcifying tooth which could be taken for such a form of dentine but that which I have designated "matrix"—that upon which the primary particles of dentine are precipitated; but it seems to me that this has no more right to be considered as dentine than the membranous border of the bones of a fœtal cranium has a right to be considered as bone.

Having now described the formation and structure of dentine, I will proceed to the consideration of enamel. The membranous matrix (see fig. 8 *m*) was described as at first single, but soon dividing into two layers—one the dentine, the other the enamel layer. The examination of the mode of formation of the enamel must be commenced from the same point, and followed in the same direction as that of dentine. The enamel is first perceptible as extremely minute bright particles, lying so near to the primary particles of dentine, and so similar in appearance, as not to be distinguishable from them (fig. 7). Soon, however, the particles of these two substances assume their characteristic differences; the dentine particles being known by their coalescing into rows of globules, or congregating in spherical masses, as has been explained—the enamel particles by their parallel linear arrangement. Sometimes the matrix is seen to divide sufficiently near to its lower border to enable the enamel particles to be distinguished from those of dentine prior to their assumption of the linear disposition, as shown in fig. 8.

The particles of enamel, after becoming disposed in dotted lines, lose much of their brightness, having coalesced into oval flat portions, which are at first separated, but which afterwards

join to form continuous wavy lines. These lines, after getting more defined and sharper, coalesce into the ordinary forms of enamel, in which all appearance of the antecedent stages becomes more or less completely effaced, or, in some cases, totally obliterated. The verification of these facts can be easily made by a careful examination of the cusps of the fœtal calf in the earliest stages of calcification; and for this purpose the portion of cusp examined should be split longitudinally into two equal pieces, one presenting the enamel and the other the dentine surface to the observer, so that they may be seen together side by side. Several cusps should be split up for this purpose, and the examination will be facilitated by the employment of glycerine.* It is scarcely necessary to say that this examination requires good illumination and great nicety of adjustment. At the commencement it will be made more easily by tracing the film of enamel backwards from the point of the cusp towards the edge of the matrix. The matrix receiving the enamel particles can generally be seen for some distance, but it gradually disappears, becoming blended with and concealed by the contiguous layers of enamel. The films of newly formed enamel soon show a disposition to break up into irregularly quadrilateral forms; but in no instance have I met with regular hexagons, as described by some authors. The laminated character of dentine and enamel will, from the explanation just given of their mode of formation, admit of being easily accounted for; the degree of its distinctness depending upon the completeness or incompleteness of the coalescence of the dentine and enamel particles, will vary in different teeth. Some occasional appearances also, such as very distinct interglobular spaces about the extremities of the laminæ; and the lines called contour lines or markings, will be explicable on the same principle; as well as the homogeneous form of enamel found in some animals, and the absence of any appreciable spaces in some parts of all teeth, the dentine being in these parts said to have no tubules, as before noticed.

The next dental tissue is the osteo-dentine or "crusta petrosa." The mode of formation of this structure can be beautifully seen in the molar teeth of the fœtal calf at the free margin of the pulp-cavity, where a thin scale of this sub-

* I have not had an opportunity of judging whether these would preserve their natural appearance if kept in glycerine for many months. But I may observe that I had a piece of oyster-shell which showed beautifully the coalescing carbonate of lime by polarized light; of which I put one piece into Canada balsam, and the other into thick glycerine—the former remains now as when first put up, but the latter, after some months, began to lose its natural appearance, and now the large globules of carbonate have altogether disappeared.

stance is found partially filling up the opening in the fang. This, which resembles ordinary bone, is formed on a membranous matrix, directly continuous with and similar in structure to that of the dentine; and the primary particles are so like dentine-particles, as only to be distinguished from them by the manner in which they afterwards become arranged. These particles appear to coalesce in the same manner, but in the place of taking a rectilinear arrangement, they have somewhat of an arborescent form, the small spicular branchings of which anastomose, and inclose areolæ of a more or less circular form. These may be regarded either as Haversian canals, lacunæ, or canaliculi, according to their size and shape, and the circumstance of their containing, or not, vessels; in which case they must of course be regarded as Haversian canals. As I have elsewhere described the structure and mode of formation of bone, I do not think it necessary to go further into this subject. The *crusta petrosa* being considered by all anatomists as bone, I have called the vessels and epithelial corpuscles in contact with its matrix "the bone-pulp," and thus the analogy between bone and dentine is preserved; the pulp-cavity of a tooth corresponding to a true Haversian canal, the spaces between the dentine rods to the lacunæ, and the extensions of these spaces between uncoalesced portions of dentine to the canaliculi of common bone. The enamel presents similar analogies, but these are much less obvious and striking.

In this paper I have, so far as I have gone, confined my observations to matters of rational inference, and such facts as can easily be verified by any one who will take the trouble, but my observations would be incomplete if something were not said of the functions of those parts which are indirectly concerned in the formation of the several structures which have been described. These are the dentine-, the enamel-, and the bone-pulps, and the part which has been designated membranous matrix. What I shall advance upon these points must of course be theoretical, and therefore to be valued only according to its degree of probability. These pulps being composed of epithelial corpuscles (I prefer the term corpuscle to cell, as there is nothing hypothetical in its meaning), and abundantly supplied with vessels, as well as containing nerves, are doubtless the organs by which the materials composing the dental tissues are elaborated. It is observed in Kölliker's 'Manual of Histology,' that the reticulated connective tissue of the enamel-pulp contains in its meshes a great quantity of fluid rich in albumen and mucus. This fact I have myself noticed. And I have

further found, when the jaw of the fœtal calf with the tooth-sacs entire within it, had been kept until decomposition commenced, that on opening these sacs the cusps were coated in parts with phosphate-crystals sufficiently large to be visible without the aid of the microscope. This circumstance is most probably due to the decomposition of some animal substance required to keep the phosphates in solution, and the subsequent dissipation of its elements in the condition of carbonate of ammonia, &c. Now, if we suppose that this albuminous fluid, holding in solution phosphates and some of the other constituents of enamel, elaborated by the enamel-organ, be applied by the ends of the enamel-corpuscles, to the external surface of the previously formed layer of enamel-matrix; and that this matrix is moistened by a fluid containing in solution a salt or salts capable of decomposing those furnished by the enamel-organ, and so combining with them as to precipitate coalescing particles of enamel such as have been described, we shall have, in principle, exactly what takes place in the formation of shell-tissue. (See this demonstrated in my work 'On the Formation of Shell and other hard structures.') As to the manner in which these organs act in elaborating their respective substances with the albumen necessary to give them their globular coalescing property is not at present known, and a more refined chemistry than has yet been applied to this branch of physiology would be required to throw light upon the subject. What has been stated in reference to enamel and the enamel-pulp will apply equally to dentine and its pulp, as also to bone, as has been shown in the article "Bone," in the volume before alluded to. As in the preceding explanation of the mode of formation of the dental tissues, no mention is made of any influence but what is chemical and mechanical, it is probable that if this paper were thus to conclude, it would be inferred that I had no belief in the participation of vitality in the several processes concerned in the production of these tissues; and thus an opportunity would be afforded of representing all that has been stated as absurd and ridiculous. Consequently a few remarks on the influence of vitality in the processes above explained will be necessary. Now, it is certain, from the foregoing account of the formation of the dental tissues, as observed in tracing their development from the condition in which they are found on first assuming a visible existence up to their completion, that both chemical and physical effects have been produced,—that new compounds have been formed is a proof of the one, and the definite forms which have

been taken up by their aggregated molecules are a proof of the other. Hence, the probable questions which will arise are,—whether these effects are entirely due to the direet and sole influence of vitality, or to the exclusive operation of physical forces? In my opinion neither of these views is correct, and one is just as untenable as the other. I am not aware that the latter has any advocates, but I believe that the former is the view generally entertained by physiologists, and that the strictly physiological part of the cytoblast theory is based upon it. To me the truth seems to be between the two extremes, the above-mentioned chemical and mechanical effects being, in my opinion, produced directly by physical and mechanical agency, but under the control of a general vital principle. According to this view, the function performed by the nucleated corpuscles of enamel-, dentine-, and bone-pulp, is chemical, each individual corpuscle being designed to elaborate a material whose elements are brought to it by the blood-vessels. Now, it is no more improbable that there should be in the bodies of animals a strictly chemical apparatus than an electric one, as in the Torpedo, or an optical one, as exemplified by the dioptrical parts of the eye. Indeed, not only animal, but vegetable structures perform an endless variety of chemical operations, and ought, therefore, likewise to abound in chemical apparatus. And it is in no way inconsistent with analogy to suppose that the nucleated particle found in the earlier states of vegetable cells is strictly an organ of this kind, and analogous to a nucleated corpuscle of the enamel-organ, and not a mere transitional form of vegetable tissue, as some suppose. This subject I hope to consider more fully in another communication in a subsequent number of the journal, showing the application of the principle of molecular coalescence to the formation and structure of starch-granules. It may be further observed concerning the influence of vitality, that if the form of a perfectly developed tooth be considered in reference to its adaptation to the place and circumstances under which it is designed and required to act, it will be at onee obvious that not only a vital, but an intelligent principle has been concerned in originating and directing the chemical and mechanical forces to which it owes its construction. And further, as the size and shape of an entire tooth depends upon the number, form, and arrangement of the several parts composing it, it is a fair inference that the same design and intelligence which originate and direct the construction of the whole, also originate and direct the physical processes concerned in the formation of the parts; and that,

not indirectly, and with the co-operation of deputed and hypothetically endowed material particles, called "nuclei or cell-germs," but by a direct exercise of power and wisdom.

DESCRIPTION OF PLATE XII,

Illustrating Mr. Rainey's paper on the Formation of Dental Tissues.

Fig.

1.—*a.* Portion of silicious cuticle from sugar-cane, as seen from above.
b. The same, from below.
c. Vertical section.

2.—Undulated margin of entirely uncalcified dentine-matrix, fom the specimen given in outline at fig. 4.

3.—Dentine in different stages of development; from fœtal calf. The lines in the engraving necessary to indicate the spaces between the dentine fibres and globules, do not exist in the specimen from which the drawing was taken.

4.—Tooth from fœtal calf in very early stage, gently removed from the subjacent pulp; *t*, tooth; *p*, dental pulp.

5.—Globular dentine; from human tooth.

6.—Horizontal section of dentine; from human tooth decalcified.

7.—Enamel in different stages of development; from fœtal calf.

8.—Matrix dividing into two layers; *m*, matrix before dividing; *e, m*, enamel matrix, with particles of enamel; *d, m*, dentine matrix.


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1c
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dm
m
5x

On the Structure *and* Mode *of* Formation *of* Starch Granules, *according to the* Principle *of* "Molecular Coalescence." By George Rainey, M.R.C.S., Lecturer on Microscopical Anatomy at St. Thomas's Hospital.

(Read before the British Association, Sept. 16th, 1859.)

Starch, from its physiological importance, remarkable structural peculiarities, and general diffusion through the vegetable kingdom, has been a favourite subject of investigation with physiologists and microscopists. However, notwithstanding the attention which has been devoted to its structure and development, it is acknowledged by the greatest physiologists to be known but little of. (See Mr. Busk's paper on "Starch Granules," in the number of the 'Quarterly Journal of Microscopical Science' for April, 1853.) There are, doubtless, intrinsic difficulties attending the investigation of this substance, but these have been very much augmented by the principle on which the examination has been conducted, namely, the cellular hypothesis. If this hypothesis had been in itself correct, and admissible as a basis of explanation of the facts connected with the structure and mode of formation of the starch granule, it ought, considering the amount of talent and ingenuity which have been employed in its application to these inquiries, to have thrown more light upon these much disputed, and as yet entirely unsettled questions.

After this apology for thus differing from the high and almost universally credited authorities of the present day, I shall proceed to explain on a new principle—one strictly mechanical in its immediate operation—"the principle of molecular coalescence,"—those points connected with the structure and development of starch granules, by which physiologists and botanists have been so long puzzled. In this paper the same train of reasoning will be employed, and the same experimental data adduced, as in my last paper, that on the "Structure and Mode of Formation of the Dental Tissues," as also in that on "Shell Structures;" and hence, though treating of a very different class of organized structures, this is still but an extension of my former researches.

It may at first appear startling that substances so dissimilar as carbonate of lime, as found in shells, or a mixture of carbonate and phosphate of lime, as it occurs in bone or dentine, should have anything in common either in their structure or in the manner in which they are formed; but I may remark that none of these structures is so simple, and so exclusively mineral or organic as is generally supposed. The carbonates and phosphates of a rounded form are all compounds of a viscid substance and earthy matter; and starch granules have diffused through their structure a small quantity of earthy matter. I have always found that starch burnt to ash on platinum leaves a residue of lime; but desirous to have more precise knowledge upon this point, I availed myself of the advantage of the assistance of Dr. Moldenhauer, the chemical assistant at St. Thomas's Hospital, in making for me a quantitative analysis of some potato-starch prepared for the purpose. The result of which is, in 100 grains of dry potato-starch—

Dry starch . . .	80·80
Water . . .	18·94
Ashes . . .	·26
	100·00

These ashes we found to contain silica and phosphate of lime; the proportions I did not think it necessary to have determined. However, the globular form of the carbonate of lime, occurring in the deep layer of the shells of Crustaceans, is as high in the physiological scale as the granules of starch.

In treating this subject I shall first consider the different forms in which the particles of starch occur, and their resemblance to corresponding forms of certain solid bodies, undoubtedly produced by the coalescence of their particles; and then I shall show that the chemical and mechanical conditions necessary to produce such forms of starch exist in the vegetable organization. The various forms of starch must be examined both when the starch is in the starch-cells and after it has been removed from them. Sections of growing vegetables in which starch is formed in large quantities, as in the very young tubers of potatoes, will serve for this purpose. In such sections, in this and the majority of plants, the starch-cells in the vicinity of the ramifications of the vessels will be seen to contain very small spherules of starch, many of them too minute to be accurately measured; yet, notwithstanding their minuteness, their figure is well defined, and they are made black or blue by iodine, proving

that they are as much starch as the larger globules, and differing from them in nothing but size. These spherules may either be free in their starch-cells, or conglomerated and joined together in pairs or threes, producing dumb-bell or somewhat triangular forms. Sometimes they are found with shreds of membrane, and at others are invested more or less by an utricle. In the starch-cells more remote, the granules are larger and fewer, so that their increase in size is attended with a diminution in number, showing most clearly that the largest are the product of the union of those of an inferior size. Indeed, the number of granules of a small size is such in some of the starch cells that it would be impossible that they all could become developed into large granules without the spaces containing them undergoing a most inordinate increase in size, which is not the fact; the spaces in which the middle-sized granules are lodged being about the same size as those containing the largest granules. But the chief evidence in support of this conclusion must be obtained from the microscopic examination of all the various forms of starch, beginning with that which is merely granular, and going up to that which is most perfect. Such an examination will show that there is exactly the same class of appearances to be found in starch, indicative of coalescence of its particles, as are presented by the several forms of carbonate of lime, whether prepared artificially or occurring in organized tissues.

Plate I contains representations of different forms of starch; fig. 1 is the ordinary form of the larger granules. This was taken from the immature fruit of the potato. Nothing that I have examined shows the laminated character of starch granules so well as these potato apples, as they are called. Figs. 2 and 3, drawn from specimens of common potato starch, are similar to those pointed out by Mr. E. J. Quekett as the result of cell multiplication by division, a view still, I believe, generally entertained by botanists. This hypothesis is considered by physiologists to apply only to even numbers, but fig. 4, and also fig. 5, which latter is copied from Crüger's plate in the 'Journ. of Micros. Science,' for April, 1854, show three granules similarly united, all as nearly as possible of the same size. Now the question is whether this hypothesis extends also to uneven numbers, or whether these specimens are merely three granules joined together, and in an early stage of coalescence. Examples of this form are not uncommon. In the specimen of starch from which these were taken there was no difficulty in finding them, being almost as common as the pairs. This starch

was prepared from potatoes which had been kept nearly a year. I have dwelt upon this form, as appearing to me rather singular that it should not have been observed by more botanists; perhaps if it had been sought for as diligently as the granules in pairs its existence would have been more generally noticed. This observation may serve as a hint in the examination of other structures in which the division of cells into two is said to take place as in cartilage. I am perfectly aware that triplets with granules of nearly equal size will, as a matter of course, be less frequent than similar pairs. Those represented in figs. 2 and 3 are a modification of the dumb-bell shape, which is seen much better in the smaller granules which unite before they lose their spherical form. These may be well seen in thin transverse sections of the very young houseleek, *Sempervivum tectorum*.

Figs. 6 and 7 are representations of a description of starch granule, called by physiologists "compound granules." These have been variously explained by different authors, but in all cases which have come under my notice the explanation of the central part of such granules has been made dependent upon some assumption which has been irreconcilable with the principle of explanation applied to the peripheral part.*

Fig. 6 is taken from Crüger's plate, as copied in the 'Microscopical Journal.' This copy, I may observe, is not introduced here, from my being unable to obtain similar specimens myself. They are frequent enough in the kind of starch called "tous les mois," but the facts very well shown by these drawings will have more weight as coming from different and independent observers. These granules consist of two or more simple granules, each having its own lamellæ, and the whole surrounded by common lamellæ.

Fig. 8 is an accurate representation of two globules of carbonate of lime from the calcifying shell of the oyster. There is so striking a resemblance between the structure of these and those marked fig. 6, that no one would question their laminated structure and their union as being otherwise than the result of a similar cause, and very likely to be produced in both cases, either by the layers of increment deposited on the inner surface of a cell-wall, or by the layers deposited around a centre or nucleus. Such was the conclusion arrived at respecting these bodies by physiologists before it was shown by me, in 1857, that exactly such forms as that represented in fig. 8 could be produced artificially, and that there were sufficient grounds for be-

* See these treated of in the April number of 1854 of the 'Quarterly Journal of Microscopical Science,' by Dr. Allman and H. Crüger.

lieving that the chemical and mechanical conditions which were employed in the experimental process for obtaining them existed in the animal organization, and therefore that both kinds of carbonate globules were of the same structure and produced under the influence of the same agencies.

Fig. 9 is a representation of some of the largest kind of artificial globules joined together, and in progress of coalescence to form a single one, just as those represented by fig. 8 are; and, doubtless, the globules of starch in figs. 6 and 7 are in a like condition of coalescenee.

I will now proceed to the second part of this paper, that is, to show that chemical and mechanical conditions similar to those in the experimental process for obtaining carbonate of lime globules, and which are necessary, on the same principle, to produce these several forms of starch, exist in the vegetable organization. This I look upon as the most novel and important part of this communication.

Now, as it is a fact generally admitted, that vegetable membrane is impermeable by solids, however minute may be their particles, it can only be in the interior of the starch-cells that starch can receive its solid form. Hence, there must exist in solution in these cavities some fluid capable of furnishing starch, or from which starch can be precipitated on the access of a second fluid containing some one or other of the constituents of starch in solution. Now, with respect to the first solution there will not be much difficulty, as dextrine—" a soluble substance found in almost all parts of plants"—or some solution analogous to it, will fulfil this— the first—condition. And as respects the second, the difficulty is still less, as there is no known solution but that of gum, which is diffused generally through plants. Hence, if starch be produced upon the principle of precipitation, from a fluid within the starch-cell, as the globular carbonate, and the mixture of globular carbonate with phosphate of lime are in the hard tissues of animals, there is no other solution but that of gum, which, from its general diffusion in the tissue of these cells, can precipitate it. Now, to show that these substances, under the circumstances they exist in vegetables, will perfectly fulfil all the conditions necessary for the formation of starch in the cells of plants, I will give some out of the many experiments which I have made for that purpose.

I will first show that gum possesses some remarkable properties which, I believe, are entirely unknown both to chemists and physiologists. One of these properties is its action as a general precipitant of substances contained in solution in the juices of plants, and the other is its action on dextrine,

from which it precipitates a modified form of starch, and its action on an alkaline solution of starch, from which it precipitates pure starch. To show the first property—that of a general precipitant—it is necessary to obtain the expressed juice of fresh vegetables, previously bruised or rasped, and filtered through blotting-paper once or twice, so that it may be perfectly clear. Some of this juice is then to be filtered into a test-tube, into which a small quantity of filtered solution of gum arabic has been introduced, when, after these fluids have remained for a few minutes, the stratum of juice in contact with the solution of gum will lose its transparency, become turbid, and soon deposit, in greater or less quantity, the vegetable matter which it had held in solution. I have performed this experiment upon the juice of several plants, and always with the same result. The juice of the bruised stems of the potato, as also that of the rasped bulbs, will serve very well for this experiment, and especially the latter, as it can at all times be procured. I may observe, that the filtered juice of some vegetables will, after standing a short time, without the addition of any gum, become turbid and deposit of itself. But this deposit I have not mistaken for that produced by the gum, the latter beginning to be apparent within a minute or two after the contact of the gum with the expressed juice, whilst the former requires several hours, or an indefinite time, for its production. I may also notice, that this property of gum is not, so far as I can discover, attributable to any earthy or metallic salt which it may contain, or to the acid which is generated by it, after being kept for some time in solution, but it appears to be essentially a property of vegetable gum, that is, of a substance which forms with water an adhesive solution, from which it is precipitated by silicate of potash, and thrown down by alcohol in the form of opaque white flakes. As, in order to be assured upon this point, I employed in my experiments gum from which the salts of lime had been separated by oxalate of ammonia, also gum which had been precipitated from its solution in water by alcohol, and after that dried and redissolved in water, also a solution of gum made slightly alkaline, all with essentially the same result as that obtained by the unpurified gum. I will now give some experiments showing the effect of gum upon dextrine, and upon starch dissolved in a solution of potash. It is well known that dextrine is formed by heating starch, and also by the action of sulphuric acid upon starch; I therefore obtained a substance known in commerce by the title of soluble gum. It is made by applying heat to starch in a suitable apparatus.

This, when put into cold water, affords a solution, which is turned brown by the action of iodine. This is a solution of dextrine. I also obtained a similar solution by mixing potato starch with sulphuric, muriatic, and nitric acids. But I generally employed that made with muriatic acid, in consequence of its not precipitating the lime from the gum, which sulphuric acid did, as a sulphate of lime, and hence did not require purified gum to be employed in the experiments with it. I employed, likewise, a dextrine made by dissolving soluble gum in water with citric acid. This I did, in consequence of the muriatic and sulphuric acids having a particular action upon gum—that of converting it into a transparent insoluble substance, which the citric acid does not.

To show the effect of a solution of gum in precipitating starch from dextrine, the same mode of experimenting as that just described in reference to its action upon the juice of plants may be employed. One way which I have found convenient to demonstrate the action of gum upon a solution of dextrine, is to put on a microscope slide a few drops of very thick solution of gum, and on the top of that a drop or two of solution of soluble gum, or of starch, acted upon by an acid, and then to examine these with the microscope whilst the action is going on, and without placing upon them any cover of glass, when it will be seen that the solution of gum causes the solidification of the dextrine starch in minute particles, having a finely granular appearance. The two solutions seem also to exert a repellant action on one another, and the starch runs into globular forms, just as oil would do if placed on water. To show this fact, and the form given to the starch, these solutions should afterwards be allowed to dry on the slide, and a drop of solution of iodide of potassium, containing also some tincture of iodine, added; and then over them a cover of thin glass may be placed. The form which the starch had taken will be seen by the colour imparted to it by the iodine. On washing these with water the circular patches of starch will be broken up, but the starch itself will remain solidified in granules of various shapes and sizes. With a view to determine how far these effects might be attributable to the medium in which the starch had been dissolved, I dissolved some potato starch in a solution of caustic potash, and, after having filtered the solution until it was entirely without any solid matter, placed a drop of it upon a solution of gum, and proceeded precisely in the same manner as described in the last experiment. I found that exactly the same effect was produced, that is, the solidifica-

tion of the starch. After these had been allowed to dry on the glass as before directed, the starch thus precipitated was transferred from the slide to a watch-glass filled with water, and allowed to remain until the gum was all dissolved, and then it was washed several times. In this case it does not alter its form, which is that of a granular areolated film of solid matter, which, from the action of iodine upon every particle of it is shown with certainty to be starch. Potash does not convert starch into dextrine like the mineral acid, but seems to dissolve it nearly, or entirely, unchanged.* The most easy way of demonstrating the effect of gum upon dextrine is to mix some solution of dextrine, made from soluble gum, dissolved in a solution of citric acid (this acid is used only to get a stronger solution) with the solution of iodine above specified, when a purple brown fluid will result, then to put a few drops of it on a glass slide close to a like quantity of clear solution of gum of considerable density. These must be made to mix under the microscope, and the effect carefully observed. The first thing which will be observable will be the precipitation of the starch in very minute granules, at first colourless, but afterwards, and almost instantly, becoming blue or dark pink. And, if the quantity of starch be considerable, the blue colour will remain for several days without changing, but, if only small, it will turn gradually pink, and so will remain unless fresh iodine be added, when it will become of a dark color. A part of the blue tint, at first produced on adding the solution of gum, is the effect of the dilution of the solution of the iodized dextrine, and can be produced by water, but in this case there is no precipitation, and, as the solution gets inspissated by the evaporation of the water, the original purple-brown of the dextrine becomes restored. For this experiment starch treated with muriatic acid, or, sulphuric will not answer in consequence of a part of the starch only being converted into dextrine, and the other being held in solution, so that when the iodine is added the latter is precipitated. When the solution contains only dextrine nothing is thrown down by the iodine.

The result of these experiments, taken altogether, shows that so completely is gum a precipitant of starch that it matters not whether it is in solution in an acid, or an alkaline menstruum, the effect is the same, although in these two cases the characters of the starch thus deposited are, as before

* When potash is employed, the lime must be separated from the gum by oxalate of ammonia, otherwise the granular film of starch will be studded with particles of the globular carbonate of lime.

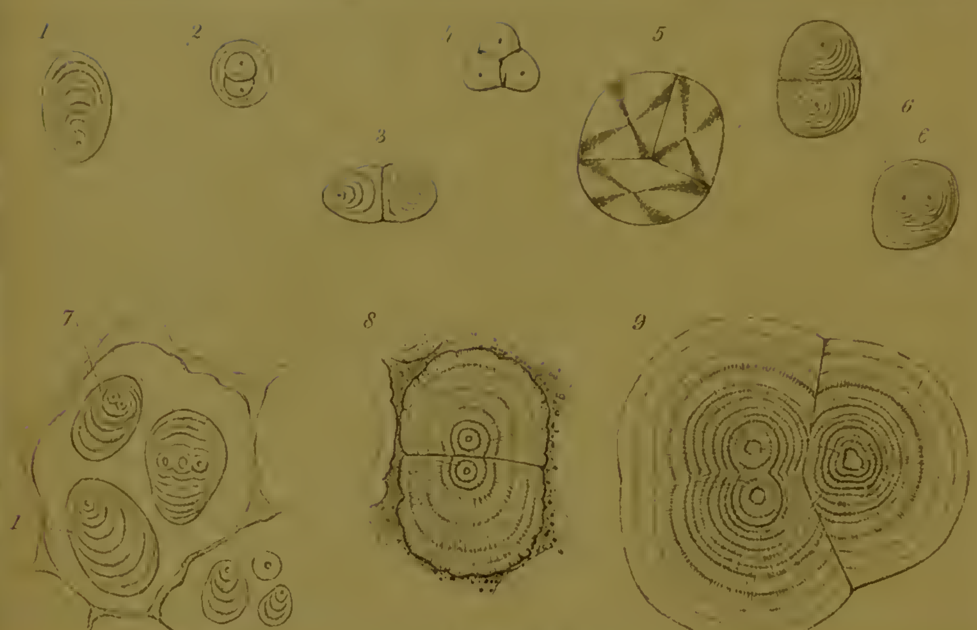
noticed, more decided in the latter than in the former. Now, gum is not the produce of any particular vegetable cells, nor is it confined to any class of plants, but it appears to be a secretion generally diffused through the tissues of all plants. Hence, combining these two circumstances, the general existence of gum in vegetables, and its property as a precipitant, I hold that one of the conditions necessary for explaining the presence of solid substances in the cells of plants by a process of precipitation is demonstrated. I need scarcely add, that the solution of gum would gain access to the fluid within the starch-cells—all by a process of endosmose. And, as to dextrine, it is generally admitted to be matter assimilated in the cells of plants for the purposes of nutrition, and therefore it is only necessary to suppose that in certain cells some such a solution of starch, as that made artificially by mixing starch and alkali together, is elaborated; (and alkali, in some form or other, is well known to be essential to the growth of plants,) and then we shall have the other condition requisite for the same process. And with such conditions there is no difficulty in seeing how a kind of modified starch, as cellulose, or the imperfect forms of chlorophylle, would be deposited in the former cells, those containing the dextrine, and pure starch in the latter. I am perfectly aware that this explanation will be considered by the vitalists as being too physical, but still it is no more so than the formation of bodies of a similar form in the shells of Molluscs and Crustaceans. The molecules of starch being thus formed and deposited, will, after repeated coalescences, produce all the forms described and represented in the accompanying plate.

The form of some of the larger starch granules may appear at first sight to have no representatives among the calcareous deposits, either natural or artificial, but this is perfectly explicable upon physical principles, and, when duly considered, is in favour of the principle of molecular coalescence. A similar difference of shape, though not to so great an extent, obtains also with the natural and artificial globules. In all the three cases the most nearly spherical forms of single globules are among the smallest, the mutual attraction of their molecules upon which rotundity depends, being less interfered with in these by the simultaneous attraction of surrounding objects than in the larger globules, as would be the case with globules of quicksilver of different sizes, placed upon a piece of glass or a sheet of paper, the smallest would be the roundest. As the particles get larger their molecules become more effectively attracted by adjacent masses of

matter, and thus the centre of attraction common to the molecules of the globule in progress of formation, and the surrounding particles of matter, towards which centre all these molecules are effectively or ineffectively attracted, cannot be the geometrical centre of the globule in question; and hence a globule formed under such circumstances cannot be accurately spherical. These conditions must always exist as well during the formation of the calcareous globules as during that of the granules of starch, but they will operate more as disturbing causes in the latter than in the former, just in proportion as the molecules of starch are less dense than those of carbonate of lime. Hence these peculiarities in the form of the large granules of starch are no more than might have been expected. The small granules of starch are, to all appearance, as spherical as those of the carbonate of lime of the same size. For an explanation of the manner in which the granules of starch acquire their laminated form, and the mode in which the hilum, the part corresponding to the central spot in the artificial calculi, is formed, I must refer to my work on the 'Mode of Formation of Shells of Animals, of Bone, and of several other Structures, by a process of Molecular Coalescence.'

I should have been glad to have introduced into this paper a condensed account of this process, as given in the volume referred to, had not the necessarily elementary character in which the process is there explained rendered the necessary abridgement of that explanation impracticable.

With respect to the chemical action of gum, and the chemical nature of the deposits thrown from the vegetable juices, I have not yet been able to make any strict investigation. I feel certain, from what I have noticed, that the subject is one of importance, and it is not impossible that it may pave the way to the discovery of similar facts connected with the action of the fluids in animal tissues. In reference to the action of gum in precipitating starch, it is not improbable that, as starch contains a minute portion of phosphate of lime, which can only have been derived from the gum, in which this salt is well known to exist, gum may furnish other constituents of starch, and also some portion of all the other substances which it has the power of precipitating, and that thus it may act both as a medium by which the various substances existing in plants are carried to the cells in which they are elaborated, and as a means of solidifying them after they have undergone the necessary elaboration.



J. Bristowe del.

DESCRIPTION OF PLATE I,

Illustrating Mr. Rainey's paper on the Formation of the Starch-granule.

Fig.

1.—Ordinary forms of starch.

2, 3.—Starch-granules; two joined together, producing an appearance considered to be the result of cell-multiplication by division.

4.—Three starch-granules similarly united.

5.—Three granules thus united, as seen by polarized light, from Crüger.

6, 7.—Starch-granules, called by author "compound granules."

8.—Two globules of carbonate of lime, joined together and coalescing into one; from calcifying shell of oyster.

9.—Large artificial calculi of carbonate of lime in progress of coalescence.

The merit of first suggesting the actual origin of sareina is due to Mr Berkeley, who stated in the " Gardener's Chronicle " (1857, Aug. 29), that he had made experiments to prove that it belonged to one of the common fungi, Penicillium or Aspergillus. This he was unable to do ; but the discovery by Mr H. O. Stephens of quaternate cells on a yellow fungus found growing on bones, rendered it highly probable that the view was a correct one.

In a communication read before the Botanical Society in 1857, I showed that parasitic fungi were derived from the two above-mentioned genera ; and singularly enough, following upon that we have the discovery, by Dr Tilbury Fox,* of sareina in a case of parasitic skin-disease. Then, in September of the present year, I found most perfeet specimens of sarcina in a phial in which I had some months previously placed a quantity of crystals of cholesterine obtained from a hydroeele.

We have thus acquired a series of links in the chain of evidence towards establishing the truth of Mr Berkeley's surmise, whieh, if not amounting to positive demonstration, is nevertheless so strong as to leave little doubt of the accuracy and justice of that gentleman's observation.

The fact is not a little interesting, inasmuch as we have now very good grounds for believing that there is no fungus which infests the human body, nor, I believe, any animal body, which is not referrible to one of the common genera, Penicillium, Aspergillus, and Mucor.

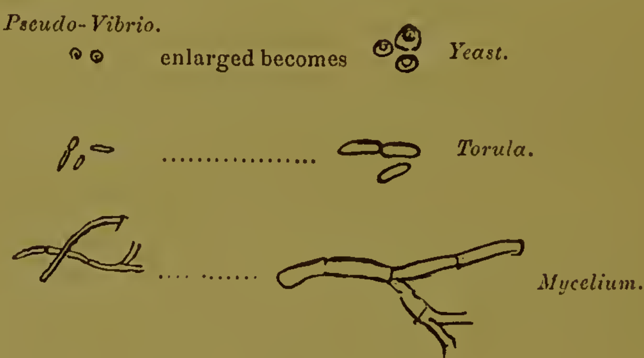
We may now consider by what means sarcina obtains ingress to the lungs and kidneys. There can be little doubt that the spores of the fungi above named are carried into the pulmonary passages during inspiration and there undergo development, and that aeeording to various modifying agencies they give rise to a mycelium which may or may not produce aërial fructification. Of the eonditions which seem to be requisite for the production of sarcina, I shall speak by and bye. To account for its occurrence in the kidney we must look for another mode of conveyance, as it is searcely probable.

* On the Identity of Parasitic Diseases, &c.—*Lancet*, September 10, 1859.

that the spores of a fungus could enter the bladder and pass along the ureters; for to effect this against the stream of urine, presupposes a locomotive power either in the spore or in the epithetial lining of the passages. We do not find either the one or the other. We must then believe that the fungus finds entrance through the circulating system, and this I regard as neither impossible nor improbable; but we have to inquire, in the first place, how the fungus obtains admission into the circulation, for it is evident that this cannot be effected in the form of sarcina, nor yet as the spore, both of which have a diameter as great, or greater, than that of blood-cells. I shall then briefly notice the means by which I think this is brought about, reserving a more extended notice which so important a subject deserves for a future occasion, after a more careful observation and investigation of facts. About two years ago, whilst examining some specimens of fungi which I had undergoing development, I found one which presented an hitherto unknown appearance. On the cork of a phial containing some ropy mushroom catsup, I observed a number of globular yellowish-white bodies about the size of pins' heads. Placing one of them under the microscope, I was surprised to find that it consisted of an innumerable quantity of non-nucleated cellules, most of which had a diameter of 7000th to 10,000th of an inch; some few being twice as large. The most minute search failed to render apparent anything like a common investing membrane. On examining the fluid in the bottle, it was found to contain a vast number of similar bodies in various stages of development. Thus, while the majority bore the same features as those on the cork, others were found to be considerably enlarged, and contained a nucleus; others, again, had assumed an oval form, and had begun to form gemmæ; whilst some had already acquired a distinctly tubular or mycelial aspect. The smallest of these cellules exactly resembled the nuclei of old yeast-cells, or what are termed by Turpin "*Globulins seminifères*," which are found in such abundance in beer at the commencement of fermentation.

I have little doubt, indeed, that they have their origin from the liberated nuclei of common fungi, capable, under certain

conditions, I believe, of undergoing division indefinitely, and of retaining the same form, but reverting to their original conformation so soon as they are placed in suitable pabula. There is nothing very improbable in this supposition, when we consider that yeast is also propagated indefinitely by gemmation and nucleation, retaining the form of yeast only whilst it remains in a saccharine fluid, but advancing to the stage of mycelium whenever the sugar is exhausted. The first change noticed from the globular form is to the oval, then to the filamentous condition. Now, just the same changes are to be observed in the progress of the cellules under consideration; they are first spherical, then oval, then linear. Increase in magnitude may go on at any of these stages. Thus the spherical may, with proper food, revert to the condition of yeast, the oval to that of the torula, the linear to that of the mycelium (*figures*). It will be remembered that these cellules



were first found on a cork, where they had doubtless taken their rise from a single nucleus. They are capable, then, of growing aerially, and thus, from their minute form, can be wafted into the air in myriads. If we examine the white powder found on old beer-barrels and on wooden utensils, wherever decaying organic matter is present, we shall find that it consists entirely of these minute bodies, which have been frequently noticed and figured as Vibriones, but which in reality are of vegetable origin. Their diameter, as before mentioned, is about $\frac{1}{10,000}$th of an inch, and some even smaller than this—not small enough to pass through a membrane, but finding access probably through slight lesions of the

capillaries or veins of the mucous surfaces. Whether this hypothesis will be found to hold good remains to be proved. I merely throw out the suggestion as one most likely to yield important results. The importance of the subject, indeed, is one which cannot be over-estimated; for if we reflect that myriads upon myriads of these minute objects are constantly floating about in the atmosphere; that they are capable of entering through the finest conceivable apertures; that their agency is purely zymotic; that bodies very closely resembling these, if not identical with them, have been found in the blood and kidneys of patients affected with typhus; if, I say, we bear in mind these facts, we must admit that there is still a great deal to be learned before we can be said to know the entire history of these apparently trivial agents.

Whether they enter the body by the channels I have pointed out, or whether by the most improbable route of the ureters, I regard it as most likely that these give rise to sarcina in the kidney; and it appears to me far from unreasonable to suppose that various zymotic diseases, if not originated, may be accelerated by the presence of these minute cellules in the blood.

Having considered the nature and origin of sarcina, we may say a few words about its relation to the disease in which it occurs. Is it merely of accidental occurrence, or is it a morbific agent in the diseases in which it is found? That there must be pre-existing disease before the parasite can be developed in the stomach, is, I think, indisputable; for in the healthy stomach, the gastric juice would certainly be sufficient to destroy any such growth. When, however, the secreting power of the stomach is impaired, or in a great measure lost, by reason of ulceration, &c., then the fungus finds a nidus amongst the diseased tissues, and in all probability tends greatly to increase the irritation of the viscus; at any rate it does so indirectly, if not by immediate contact, and this by virtue of its power of exciting fermentative decomposition, the products of which, by distending the stomach, and by their irritant action, cause frequent efforts at vomiting, and give rise to the yeasty appearance of the ejected contents.

Finally, as to the reason of the plant continuing to grow in what appears at first sight to be an unsuitable locality. This, I have already stated, is in a measure owing to a previously vitiated state of the lining membrane of the stomach; but there is evidently some special food which it meets with, and which it finds in but few other localities, serving to retain it in the state of sarcina. Indeed, I regard it as essential to its development, that this peculiar pabulum should pre-exist. In what, then, does this peculiarity consist? The specimen of cholesterine crystals in water, which gave rise to sarcina, I found to be most intolerably fetid, from the disengagement of hydrosulphuret of ammonium. Mr Stephens finds his specimen of the plant on bones. In other cases it occurs on diseased tissues, the decomposition of which would yield some such gas as the above. May not this, or a similar gas, be the food requisite for the production of this peculiar form of the plant? It seems to me to be not improbable, and that on the exhaustion of this supply it returns to its pristine form, just as yeast acts, after the failure in the supply of sugar.

However far these suggestions may be found to hold good, it admits now of scarcely a doubt that sarcina is neither more nor less than an algal condition of a common fungus. Mr Berkeley, indeed, speaks of it as being the spore of the plant. With much diffidence, I venture to express an opinion at variance with that of so excellent a mycologist.

It seems to me that the term spore is often loosely and vaguely applied to small cryptogamic cells, whose origin and purpose seem to be obscure. The term ought, I think, to be confined entirely to those bodies which are the result of a true reproductive process. There is, so far as I am aware, no observation to prove that sarcina is so produced; and we ought therefore to avoid giving it an appellation which is calculated to originate an erroneous impression of its nature. We have much to learn as yet regarding the reproduction of fungi, and it will, I believe, be found eventually, that the fact of cells undergoing segmentation is entirely opposed to the view of their being spores or true reproductive cells. Looking at it in this light, it seems quite contrary to experience, and

all our ideas of sexual reproduction, to imagine that the ovum may go on dividing itself into millions of other ova, each capable of producing the mature plant. We should thus have, as in the instance of yeast, many millions, nay, even millions of millions of plants arising from a single ovum. From the analogy observable in other cryptogamous plants, we may, I think, assume it as a fact, that each true reproductive cell can give rise to only a single mature individual, but that a single plant may give rise to endless gemmations. And, as a corollary to this, I would add, that where gemmation, or, what is the same thing, fissiparous division, exists, there is no reproductive process, and *ergo*, the results are not true spores.

From this it follows, that yeast is nothing more than a gemmation of the fungus. True, it is derived from the so-called aërial spores of the penicillium, &c., but these are, I believe, in reality gemmæ, just as the *spores* of a fern are. The true reproductive organs exist in the mycelium. So with sarcina, whose fissiparous division is nothing more nor less than mere budding. And so with other fungi, which are propagated in like manner.

These views on so obscure a subject are not put forth dogmatically, but merely to excite inquiry into a subject which is surrounded by much that is interesting. That they will bear investigation, however, I fully believe, since there is no statement made which is not borne out by analogy in other cryptogamic families.